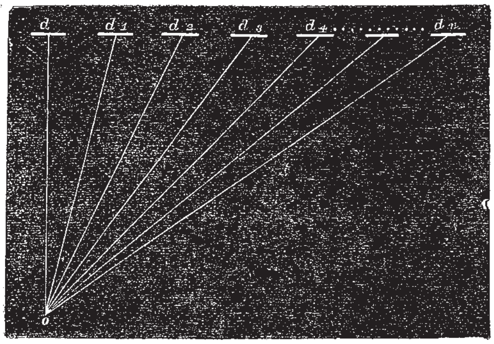
$\sqrt{D^2 + 9\delta^2}$; the reflected sounds which reach the observer will travel double those distances.

$D\ (D^2 + \delta^2)\ (D^2 + 4\delta^2)$ &c., being integral quantities, and δ positive, the series will be an increasing one; hence the first impulse which is heard is that produced by d, and the last one that by d_n.



Twice the difference between any term and that which immediately precedes it will be the length of the sound-wave corresponding to that term, and the velocity of sound per second, divided by the wave-lengths, gives the relative pitches of the different impulses.

The wave-lengths corresponding to $d\,d_1\,d_2$ &c. are—

$$2(\sqrt{D^2 + \delta^2} - D);\ 2(\sqrt{D^2 + 4\delta^2} - \sqrt{D^2 + \delta^2});$$
$$2(\sqrt{D^2 + 9\delta^2} - \sqrt{D^2 + 4\delta^2})\ \text{\&c.} \qquad (1.)$$

And calling V the velocity of sound per second, we get the relative pitches—

$$\frac{V}{2(\sqrt{D^2 + \delta^2} - D)};\ \frac{V}{2(\sqrt{D^2 + 4\delta^2} - \sqrt{D^2 + \delta^2})}\ \text{\&c.}$$

Now, if the observer removes close up to the fence, the distance D becomes an indefinitely small quantity, or zero, and the series (1) for the wave-lengths becomes $2\sqrt{\delta^2}$; $2(2\sqrt{\delta^2} - \sqrt{\delta^2})$; $2(3\sqrt{\delta^2} - 2\sqrt{\delta^2})$; $2(4\sqrt{\delta^2} - 3\sqrt{\delta^2})$, &c., or 2δ, 2δ, 2δ, &c.; that is, the wave-lengths are all equal, and a musical sound is heard. In practice, an ordinary fence does not yield a sufficiently loud note to be easily heard in this case, but one made with posts having intervening spaces of about five inches gives a good result when one stands four or five feet from it, the note comes out almost perfect. By taking different values for D we have from series (1) a corresponding change of wave-lengths, so that if a row of persons are placed from o to d, each will hear a sound which is different in pitch from that heard by all the others.

It is perhaps needful to state that the sound which has been described is completely masked if there are houses or a wall a few feet behind it, or if the place of observation is a road fenced with palisades on both sides, two sounds are produced which interfere and confuse each other.

Glasgow ANDREW FRENCH

The Degeneracy of Man

THE numbers of NATURE for June and July last, which have lately reached me (vol. x. pp. 146, 164, 204 and 205), contain a correspondence on the subject of the degeneracy of man, in connection with which I wish to contribute a few remarks.

I have nothing to say on the original point introduced by Mr. E. B. Tylor. But, during my residence in the islands of the Pacific, I have given some attention to the general question of degradation or progression, as exhibited in the Polynesians. The result is, that I believe there are numerous indications of the degeneracy of these people from a higher social and intellectual level than that which they at present occupy. I could not give in detail, in this letter, the entire evidence on which this opinion is based; I will therefore briefly mention two or three indications only of this degeneracy which I have noticed.

The language of the Polynesians furnishes one of these. While there is much in it which shows a low moral tone, there are, on the other hand, many refinements (a large proportion of which are known to most of the present generation) which I do not believe could have been invented, or gradually developed, by the race in its present intellectual condition. Their old traditional stories, and their ancient poetry also, are so different from anything the present Polynesians are capable of producing, that I often think (your classical readers will please pardon the comparison) the relative difference, between the past and present, is as great as that between the intellect of the Greeks, in the period of the highest Attic culture, and those of the present century. I have often asked men of more than average intelligence, why their modern compositions are so inferior to many of the old ones. They invariably reply that the men of old were greater and wiser than those of the later generations.

The industrial and ornamental works of the Polynesians are all, I believe, of ancient origin. Their houses, their canoes (with one exception), their fine mats, the way in which they make their bark cloth, and even the patterns which they print on it, are all according to the traditional forms handed down from generation to generation. There is no originality. Invention is unthought of. Even now, when the influence of external civilisation is brought to bear with considerable force upon them, they adopt a new idea very, very slowly. If they had never been in a higher and more active intellectual condition, I cannot conceive how they could possibly have obtained the many comparatively excellent customs, the—in many respects—elaborate language, and the advanced social customs which were in their possession when first they became known to the civilised world.

I am well aware that absolute proof of the degeneracy of the Polynesians will not, by any means, render necessary the conclusion that degeneracy has been universal with the human race. Advocates of the progressive theory do not deny that some instances of degradation are to be found. In his "Primitive Culture" (vol. i. p. 34) Mr. Tylor says: "Of course the progression-theory recognises degradation, and the degradation-theory recognises progression, as powerful influences in the course of culture." Hence I present the indications of degeneracy above-mentioned as, at most, only a minute portion of the cumulative evidence which must be adduced indisputably to prove the degradation-theory of general application to the human race.

Apropos of this question I may add, that I often think much of the difference between (at least the more moderate) progressionists and degradationists is owing to the want of a clear definition of the term *civilisation* as used on either side. One appears to me to think chiefly of a *material civilisation*, while the other thinks mainly of a *moral civilisation*. I do not believe in the evolution of man from a lower form of life. But, notwithstanding this, I doubt whether the first man was *civilised* in the ordinary sense in which that word is now used. So far as a material civilisation goes, I take him to have belonged to the earliest stone age. But at the same time I feel the strongest conviction that he was, in point of moral civilisation, immeasurably in advance of a savage. It has often been said by advocates of the degradation-theory that no well-authenticated instance has ever been given of a savage who has, apart from external help, improved his condition. I believe this assertion to be true, notwithstanding Sir John Lubbock's "Cases in which some improvement does appear to have taken place," given in the appendix to his "Origin of Civilisation" (pp. 376-380). I do not deny the force of the reply to the above assertion, given by advocates of the progression-theory; viz., that it is almost impossible to *prove* that a savage race has, unaided by external influence, bettered its condition. But from personal observation of savage and semi-savage life, I feel almost certain that a real savage is utterly incapable of, in any way, raising himself. He lacks the sensibility which must serve as a fulcrum for the lever which is to lift him. Upon this ground alone, if I had no other reason for it, I should doubt whether man had, unaided, developed himself from a state of unmitigated savagery.

Upolu, Samoa S. J. WHITMEE

The Law of Muscular Action

IN NATURE vol. xi. p. 426, my esteemed friend Prof. Hinrichs does me the honour to comment on my paper published in NATURE, vol. xi. pp. 256 and 276.

He claims to have found that in lifting a weight w until exhaustion sets in, the number of lifts n is represented by the equation—

$$\left.\begin{aligned} n &= \frac{A}{B^w} \\ \text{or} \qquad \log. n &= \log. A - w \log. B_1 \end{aligned}\right\} \qquad (1)$$

where A and B are constants.

That the relation between n and w (the strength of the muscle

remaining constant) is a logarithmic function was plainly indicated in the last paragraph but one in my second paper in NATURE, p. 277. In my paper in the *American Journal of Science*, Feb. 1875, p. 130, a formula was given at the close of the paper, p. 137, which is equivalent to Hinrich's formula (1), calling τ the time of exhaustion (or number of lifts), and s the strength of the muscle obtained with a dynamometer, and

$$\tau = a(s - \beta)^v \quad (2)$$

where a and β are constants. If the dynamometer gave the real strength in kilograms, β would equal w. In the series published in NATURE, s was obtained in another way, there described, and β was zero (nearly); v is a function of the weight. So that Hinrichs' formula does not seem to differ essentially from (2). In giving this formula, I stated expressly that I did not wish to discuss this equation at present, as the constants had not been determined with satisfactory precision. I take this occasion to repeat that statement.

Another point to which it may be well to call attention is, that in exhausting the arm with heavy weights very little pain is felt. With light weights, however, the pain is very great.

Our knowledge of this whole subject is yet so fragmentary, and the subject itself is so complex, that we can only hope to represent our knowledge by empirical formulæ. The best service is to be rendered in the direction of careful experiment. I shall therefore devote a few years to the work outlined in my paper in the *American Journal of Science*.

Washington University, F. E. NIPHER
St. Louis, Mo., April 28

Physiological Effects of Tobacco Smoke

Is Dr. Krause (NATURE, vol. xi. p. 456, vol. xii. p. 14) acquainted with the manner in which cascarilla bark modifies the physiological effects of tobacco smoking? The addition of a few very small fragments of the bark can hardly be supposed to materially affect the amount of carbonic oxide produced; and yet, with such an admixture, the strongest tobacco may be smoked by a tyro without, in most cases, the production of the usual nauseating effects. Loss of appetite, thirst, vascular and nervous depression are sometimes produced if such a mixture is smoked in excess. On the other hand, if Dr. Krause's theory, that the nausea, &c., of tobacco smoking is due to the carbonic oxide inhaled, be admitted, the question is suggested whether some of the volatile products of burnt cascarilla bark are antagonistic in their physiological action to the gas in question? C. E. S.

OUR ASTRONOMICAL COLUMN

NEW VARIABLE STAR (?).—Mr. J. E. Gore, of Umballa, writes with reference to a star of about the 6th magnitude noticed on the 13th of January about 1° north, following θ Leporis, and not having found it in Harding's Atlas or in Lalande, or the B. A. C., he supposed it might be a new star. "It is of a reddish colour, and is in the same low-power field with, and about 25′ north of (a little preceding) the 7m. star Lalande 11778 . . . It is closely followed by two small stars which formed with it a curved line." From this description the star is evidently VI. 58 of Weisse's first Catalogue, observed by Bessel early in 1825, and estimated 6·7 magnitude, the small stars preceding it being Nos. 68 and 78 of the same hour. It is not found in D'Agelet, Lamont, or in any other catalogue we have examined, of previous date to that accompanying Heis's Atlas, where it is entered 6·7, but erroneously identified with VI. 78 of Weisse's second Catalogue, instead of VI. 58 of his first. (The large number of similar errors in Heis's references is a serious defect in a work otherwise of so much value.) Mr. Gore mentions that he had not remarked, up to the middle of April, any variation in the star's light, but it evidently requires further examination, and may yet appear on our rapidly extending list of variables.

THE BINARY STAR ζ HERCULIS.—If good measures of this star are obtained during the present season, we may expect to know the elements of the orbit with considerable precision. Dunér's results, founded upon measures 1826–69, will be the best so far published, but he did not regard them as definitive; they will no doubt be very useful in any further investigation, and for this reason are here subjoined:—

Peri-astron passage 1864·23

Node	45° 56′	Excentricity ...	0·42394
Node to peri-astron on orbit ...	250 50	Semi-axis ...	1″·223
Inclination ...	34 52	Period	34·221 yrs.

PETERS' ELLIPTIC COMET 1846 (VI.).—This comet, which was detected at Naples on the 26th of June, 1846, by Dr. Peters, now Director of the Observatory at Clinton, New York, was calculated by Prof. D'Arrest, and in a more complete form by the discoverer himself, who, in a memoir published in the Transactions of the Naples Academy in 1847, found the time of revolution 12·85 years, but with an uncertainty of ± 1·61 years; in a subsequent communication to Brünnow's *Astronomical Notices*, he gave elements for 1859, including the effect of perturbations of the planet Saturn, which, however, he shows to be liable to very considerable doubt, on account of the observations in 1846 being insufficient to fix the mean motion at perihelion in that year within narrow limits. It is to be remarked that in 1846 the comet appeared under nearly the most favourable circumstances possible for observation, and at the time of discovery the comet was distant from the earth less than 0·6 of our mean distance from the sun, yet Dr. Peters found it very small and faint, and unless the perihelion passage should happen to fall about the same time of the year as in 1846, it might be exceedingly difficult, if not impossible, to recover it. The only hope of doing so is in keeping a close watch in the late spring and early summer, upon those parts of the sky indicated with different suppositions for date of perihelion passage, say from May 15 to June 15, which are wholly in south declination, a circumstance that will render the assistance of observers in the other hemisphere very desirable. To give an idea of the comet's track in the heavens when the perihelion falls in May, we assume the 15th and 25th for the passage by this point of the orbit, and thus have the following positions:—

	In perihelion, May 15·0.			In perihelion, May 25·0.		
	R.A.	Decl.	Distance	R.A.	Decl.	Distance
May 15	256°·5	50°·0 S	0·594	228°·8.	55°·8 S	0·600
„ 25	256 ·5	42 ·2	0·552	231 ·1	48 ·4	0·561
June 4	255 ·9	32 ·8	0·538	233 ·4	39 ·2	0·546
„ 14	255 ·3	23 ·1 S	0·555	235 ·8.	29 ·4 S	0·564

The least distance between the orbits of the earth and comet is about 0·53.

Considering the uncertainty in the mean motion deduced from observation in 1846, it is quite within possibility that a perihelion passage may occur as late as the summer of the present year, and it may be worth while to institute a search upon that supposition.

MINOR PLANETS.—No. 26, *Circular zum Berliner Astronomischen Jahrbuch*, just issued, contains new elements and an ephemeris of No. 114, Cassandra, and corrected ephemerides of No. 71, Niobe, and No. 128, Nemesis. The period of revolution assigned to Cassandra for November 1872 is 1598·5 days. Several of this group are now adrift, the elements not having been determined with sufficient approximation to keep them in view. The planet found by Borrelly at Marseilles, 1868, May 29, and that detected by Pogson at Madras on November 17 in the same year, are thus situated; both travel beyond the limits of our ecliptical charts, which contain very small stars.

OUR BOTANICAL COLUMN

THE PANDANEÆ.—A fine series of Pandanus fruits has recently been received at the Kew Museum from Mr. John Horne, of the Botanic Garden, Mauritius